

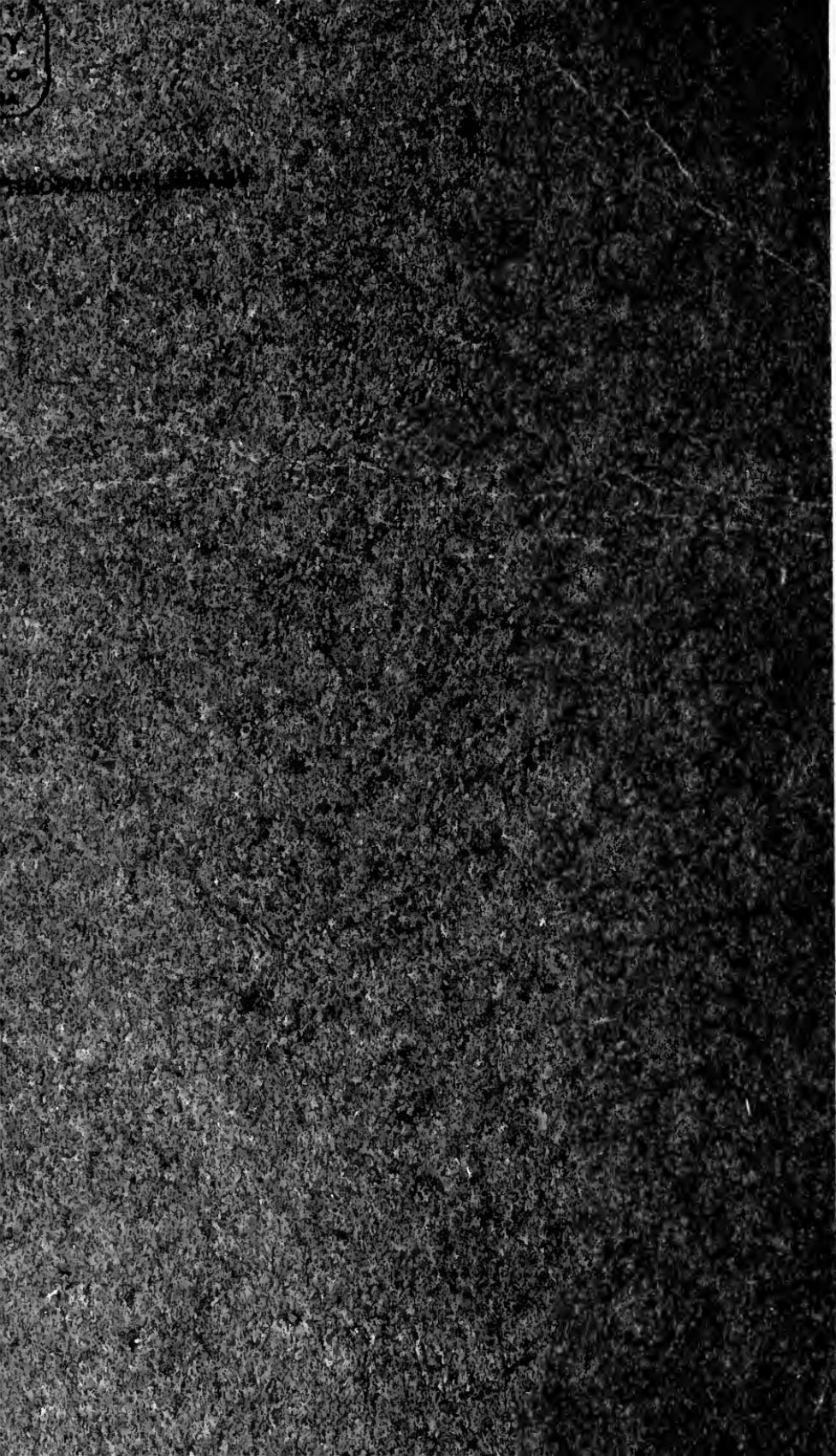
GN  
6  
H37

UC-NRLF



B 3 424 818

UC LIBRARY







*tc. to g.* *Hay, Olin - (no. 1-5)* *Per. 4* *hd 5 nos. tog.* *new*

No. 1—pp. 1-4

September 10, 1919

## ANTHROPOLOGIC SCRAPS.

1211 HARVARD STREET,

WASHINGTON, D. C.

*GN 6*  
*H 37*

### A STUDY OF SKULLS AND PROBABILITIES.

In 1887 a workman, in digging a grave on one of the highest knolls of Riverview Cemetery, at Trenton, New Jersey, found a human skull at a depth of about 3 feet. This skull was given to Mr. E. Volk and by him was turned over to the Peabody Museum at Cambridge. Mr. Volk examined the grave and found no evidences that the materials overlying the skull had suffered any previous disturbance. The grave digger observed in the soil only a few black lines. It was at once recognized by Prof. F. W. Putnam that this skull was very different from any of the Indian skulls found in that region. Accompanying it were no other bones whatever. It becomes a problem to the geologist and anthropologist to determine how that skull got there and its geological age and racial relationship; and when they have done their best the solution will be reached by a balancing of probabilities. Scarcely one fact in the case is absolutely certain.

Probably the judgment of the great majority of persons would be that a skull found thus alone in a loose deposit had become separated from the rest of the skeleton and been swept along with the sand and gravel by water or whatever agency had deposited the earthy materials. This general opinion might be regarded as a measure of the probabilities in the case; but it would not be necessarily correct. Might not, therefore, the skull have been intentionally buried there, with or without the rest of the body, at some time nearby or far away? The fact that no disturbance was noted in the loam and sand and gravel appears to furnish probabilities against such a burial, for such disturbances persist for an indefinite time. In case the idea of a purposive burial is still entertained, one must take into account the chances that all the other bones of the body might decay utterly and leave the skull in fair condition. Even the lower jaw, one of the most persistent bones, was gone. Also all of the upper teeth must

have completely rotted in their sockets, while the alveolar processes in part remained. Although the compact limb bones and the teeth were gone, the thin bones of the skull, even some of those of the nasal passages, remained intact.

If the skull belonged in the yellow loam of that locality and not in the Trenton gravel, we possess some facts bearing on the case. Volk found at the bottom of this loam remains of several skeletons, and these have been studied by Dr. Ales Hrdlicka (Bull. Amer. Mus. Nat. Hist., Vol. XVI, 1902, p. 46). Of five crania only a few pieces were preserved, all too small to be of any use. A number of long bones, however, furnished measurements and notes. These deep and evidently old burials do not, therefore, appear to favor the probability that the Riverview skull was a recent inhumation.

Now as regards the geological age and racial relationships of the Riverview man. Inasmuch as for 40 years it has been contended that the Trenton loam contained evidences of a culture different from that of the modern Indians and much older, it was natural that some bold, possibly rash, spirit would be led to connect, at least provisionally, the peculiar skull with the supposed ancient race. Doctor Hrdlicka permitted himself to do this (op. cit. p. 57). He concluded that, in case the skull was not intrusive, the problem became one almost wholly of geology.

In 1907 (Bull. 33, Amer. Ethnol., pp. 36-46) Doctor Hrdlicka, then come into the Government service, returned to the subject, having meanwhile made a discovery. From the literature of anthropology he had learned of the occurrence of a number of extraordinarily low skulls along the northwestern coast of Germany and Holland. In lowness and some other features these approximated to the type of the Neanderthal cranium. To this conformation of skull is applied the term *chamaecephaly*. Doctor Hrdlicka concluded that close kinship existed between the European and the New Jersey specimens, that the type of skull is very old, and that the American representatives may have traversed the Atlantic Ocean many thousands of years ago. However, he went on to say that the probabilities are against the ancient origin of the Riverview skull; and he proceeded to draw on the history of the region. It had been peopled largely by Swedes; but the skull was not that of a Swede. Among the Swedes there were found to be some immigrants from Holland, "among whom were very likely individuals of the low cranial type." He concluded that the deposits in which the Riverview skull was found do not preclude a

*Handwritten Note*

comparatively recent burial and that it seemed safer and more in line with known evidence to regard the skull as of relatively modern and European origin than as a representative of Quaternary man.

For those who do not care to have their conclusions handed to them ready made there is offered here a fine exercise in the balancing of probabilities.

First may be considered the question whether or not any of the Dutch chamaecephals really came to the new settlement at Trenton. It is possible, indeed, that all such individuals in Holland and Germany reached that place. This would easily explain the fact that none appear to have been recognized since that time in the formerly infested European region. It would further make it probable that a considerable number of chamaecephals might now be found in the population of Trenton, especially among the descendants of the old families. On the other hand, one might insist that there is a strong probability that none whatever of these low-browed Dutch came to the region; and this view might be supported by noting the probable trifling ratio of Dutch chamaecephals about 250 years ago to the number of Dutchmen possessing normal domes. The present writer, however, wishes to be generous, and he grants that as many as two of these persons with depressed cranial vaults may have wandered to the pleasant banks of the Delaware.

Let us suppose now that in 1887 some shrewd gambler (or even the reader) had been told that several hundred years ago men existed in Holland who had chamaecephalic skulls, that some of their descendants probably existed up to about 200 years ago, and that very likely individuals of these came to New Jersey; that, further, he is invited to wager a sum of money that the newly opened grave in Riverview Cemetery belonged to one of these defectives and that his identity would be established. When the chances are even, odds can hardly be demanded; but what odds might be required on such a proposition? Suppose further that he is asked to wager that of the skeleton every shred had disappeared except the well-preserved skull minus the lower jaw; how many fold must the odds have been increased? If, when the question of racial identification came up, the chamaecephalic skulls discovered in Holland had been laid out on one side and the Indian skulls of low type described and figured by Hrdlicka (Bull. 33, p. 99, pls. xiv-xxi) on the other, as standards of comparison, would the aleatory venture have appeared more or less promising? Would not our prospective investor in chances have wanted

to consult a board of experts, and besides have wished to know something about the anthropological prejudices of the stakeholder?

Suppose now that our man has made up his mind as to the odds that he will demand to balance his risks, and he then happens to learn the facts about the remains found near Sykesville, Burlington County, those of a second chamaecephal who may have come to New Jersey. Would he have regarded it as the most natural thing in the world that two such rarely endowed personages, old companions in exile, should, after two centuries of repose, happen to come forth from their graves so nearly at the same time; or would he look upon it as quite improbable? Let him further learn that in the Sykesville case, as in the one he was thinking of betting on, no lower jaw was secured and no other bones except the moderately preserved skull. Would he conclude that two complex combinations of that kind were likely to come to pass? Would he demand then simply that the odds be again doubled or that they be multiplied by  $2^n$ ?

The writer will not attempt to answer these questions. It is for each reader to determine for himself where the figure ought to be placed between even money and a sum that would tax resources superior to those of the luxurious Count of Monte Cristo. But the reader has also the privilege of suspecting that the Riverview skull and that from Sykesville are two out of many thousands, chamaecephalic or normal, that were buried, intentionally or by accident, in that region many hundreds, it may be thousands, of years ago; and that of these, without any miracle, others in due time will come to light. The identification of the skulls as those of expatriated lowbrows from the Zuyder Zee may be looked upon kindly as a well-meant effort to bring somewhat tardy relief in a case that was critical.

OLIVER P. HAY.

No. 2—pp. 5-8.

December 3, 1919.

## ANTHROPOLOGIC SCRAPS.

1211 HARVARD STREET.

WASHINGTON, D. C.

---

### ON PLEISTOCENE MAN AT TRENTON, NEW JERSEY.

---

The writer sees fit to continue the discussion of the presence of Pleistocene man in North America.

More than 40 years ago the late Dr. C. C. Abbott called attention to the occurrence of rude argillite artifacts in the sandy loam of the high terrace at Trenton, New Jersey. His opinion was that the objects represented a culture far older and more primitive than that of the modern Indians. Opposition was not slow in presenting itself; and the ensuing dispute culminated in the debate at the Detroit meeting of the American Association, in 1897 (Proc. A. A. A. S. Vol. XLVI, pp. 344-390). That discussion furnishes reading that is even yet interesting and instructive.

The contestants were about equally divided in numbers. There was agreement as to the presence of three deposits, the upper black soil about 1 foot thick, the yellow loam 2 feet thick, and the underlying Trenton gravel; also as to the glacial age of the gravel. The battle was over the yellow loam, its age, the agencies that had laid it down, its subsequent modifications, and the origin of the included artifacts. The believers in the existence of glacial man insisted that the loam had been deposited while the glacial front was yet not many miles away, by the waters of the Delaware, flowing yet, at least during floods, at the level of the terrace; that the loam had suffered little subsequent disturbance; and that the artifacts had been included at the time of deposition of the loam. The opposition contended that none of these propositions were supported by sufficient evidence. The loam, they argued, might have been laid down as asserted, or at a later time by marine waters, or by the action of winds. The artifacts might have been included at the beginning or introduced at any subsequent time. However the loam had come there, it had later undergone extensive derangement. Disturbances had been produced by freezing and thawing, by the penetration of roots of trees, by the

uprooting of trees during storms, by the drilling occasioned by insects and worms, by the burrowing of mammals, by the digging of graves, by the planting of palisades, by the crumbling of banks, and like causes. It was denied that the artifacts had been produced by a people other than the ancestors of modern Indians.

These dissenters presented no statistics to show how many trees are uprooted on a square mile in that region during a definite term of years and what effects are thereby produced. Nor did any one calculate how many thousands of years it would require for an average population of woodchucks, the most assiduous and extensive burrowers, to turn over that loam and mingle with it the stones of the underlying gravels and the artifacts of the overlying soil. He might have given himself a large surprise by a minimum of figuring.

Now, has that loam really been disturbed to any appreciable extent? We may gain information from two sources. The formation is traversed by 3 or 4 red bands which vary much in thickness, but which are very persistent. At the Detroit meeting there was a pronounced difference of opinion whether these represented planes of stratification or bands of segregation, later produced. Which explanation is the true one is for our purpose of little consequence. The bands certainly had not been formed suddenly or recently. If all the disturbances that have been invoked had occurred there, those bands, if of stratification, would long ago have been broken up; or, if of segregation, could never have come into existence. Their condition of practical continuity is evidence against the theory of extensive disturbances.

Testimony was rendered at the Detroit meeting by at least two geologists, and it has been confirmed by others, that in nearly all cases the artifacts are found lying horizontally on their flat faces. How was it possible for these objects to retain their horizontality if the ground had been so thoroughly worked over as claimed? How could they have assumed and retained so generally this position, in case that they had made their way from the surface by way of gopher holes and holes produced by the roots of trees? And, by the way, it would have been a valuable contribution to geology, and to anthropology likewise, if some one had explained just how tree roots function in introducing objects from the surface into deep soils, and to what extent they effect this.

Now must be considered the results of the investigations instituted by the American Museum of Natural History, New York, as reported by Mr. Leslie Spier and Dr. Clark Wissler (Amer. Anthropologist, Vol. XVIII, 1916, pp. 181-197). In running their trenches the ex-

cavators noted the exact position of each pebble and of each artifact, and the results have been tabulated and illustrated graphically. Both the pebbles and the artifacts were found to be not indiscriminately scattered. They are most abundant along a plane about the middle of the loam. Downward, they diminish in numbers to a barren zone; upward, to a nearly barren zone just below the black soil. How can those who promote the theory of disturbances and adventitious introductions explain this apparent attraction of the median plane for objects both ascending and descending? In an important work just issued (Bull. 60, Bur. Amer. Ethnol., p. 77) and dealing largely with the antiquity of man in America, the old view is revamped; and there is not the slightest allusion to Spier's and Wissler's subversive studies. Furthermore, if that loam, with its gravel, is of aeolian origin we have there a remarkable record of a secular beginning, culmination, and decline of high winds. And, mind you, the vertical distribution of the artifacts coincides with that of the pebbles. Must we therefore conclude that a race of argillite men, starting from a feeble stock, increased, flourished for a while, and then *pari passu* with the winds, degenerated and disappeared? It is hardly to be supposed that the boulders found in the loam were blown there. Is it possible that they were carried there by the argillite men as ballast?

The reports alluded to show, further, that the artifacts, probably also the pebbles, are not evenly distributed horizontally, but occur in patches, toward the center of which they are most numerous. How are our good friends who believe in the efficacy of adventitious agencies going to explain this distribution? An eddy in a stream would produce just that result. In such a swirl pebbles and artifacts would tend to be drawn toward the center. If the conformation of the river bed and of the shore were such as to produce a succession of eddies, and if the current were at the same time attacking a bank containing pebbles and artifacts, these would probably be distributed as they are at Trenton. Out of such a bank may have come also that Riverview skull, the relic of the man who has been made to pose as a chamaecephalic Dutchman.

The terrace at Trenton is spread out as a great experimental farm where the adventitious agencies have had full play. The theory that these have done the work attributed to them has been tested. A more complete confutation of that theory could hardly be desired or feared. That theory, which has been, as it were, standardized since the Trenton dispute began, has been applied to nearly every case in which the presence of Pleistocene man has been suspected. Only

recently (Jour. Geology, Vol. XXVIII, pp. 305-338) our great and honored geologist at Chicago has applied it with additional rigor to the discoveries at Vero, Florida. At this stage, however, the theory, discredited at its place of birth and of admittedly doubtful applicability in the great majority of cases subjected to it, appears to require extensive modifications, restrictions and reservations. One may view with tranquil mind the outcome of the situation at Vero.

If, when the eminent geologist referred to undertook to decipher the history of the glacial period in North America he had imposed on himself the solution of every doubtful problem that presented itself, had listened to the cries of those who feared that he would sacrifice his scientific reputation and would do violence to common sense, religion and science,—if he had on these accounts hesitated, he would have accomplished little. He had, however, the sagacity to discern the essential features of the problem and to draw conclusions from them, and to leave the minor difficulties to the future. He has thereby made a notable contribution to human knowledge.

Some of the perils attending attempts at prophecy are illustrated by the debate at Detroit and by subsequent events. Along the cut of the Pennsylvania Railroad in Trenton many artifacts had been found which purported to come from the Trenton gravel. One of the speakers, author of very much meritorious work, insisted that the deposits had been reworked by the nearby stream; and that, since such reworked deposits had been removed, nothing for 5 years had been found. All then was at an end for the pretended glacial man. Two years passed, however, and patient, watchful Ernest Volk brought out of the gravels a piece of human femur and a part of a skull. Does the author of Bulletin 60 admit that, on his own showing, those bones decide the case against him? Unfortunately one finds no such concession.

If anthropologists should feel themselves compelled, as indeed some are feeling themselves compelled (Nelson, in Nat. Hist., New York, Vol. XIX, p. 139), to admit that human beings were living at Trenton at the close of the glacial period, they can not stop there. Neither red Indians nor argillite men could probably have made their way from Asia over the great wall of Wisconsin ice that stretched from the Atlantic to the Pacific. They must have been in North America during the Peorian interglacial. If anthropologists are driven to accept this view they will then have set their feet on a path that slopes down steeply and inevitably to the early Pleistocene.

OLIVER P. HAY.

No. 3—pp. 9-12.

March 29, 1920.

## ANTHROPOLOGIC SCRAPS.

1211 HARVARD STREET.

WASHINGTON, D. C.

---

### BULLETIN 60, BUREAU OF AMERICAN ETHNOLOGY.

---

Let no one suppose that, in penning these articles on the antiquity of man in America, the writer is inspired by ill-feeling toward any one who holds opposite views. No such sentiment is entertained; not even toward the anthropologist who would try to run in as changelings, for one Pleistocene man a base-born Caucasio-Amerind and for another an astray chamæcephalic Dutchman. I honor my scientific confreres and desire greatly to turn some of them from the errors of their ways.

The Bureau of Ethnology not long ago issued its Bulletin No. 60. The author is the head-curator of anthropology in the U. S. National Museum; the major subject is the lithic industries of the aborigines in America. So far as the present writer can judge, the principal theme has been elaborated by the strong and steady hand of a master. In an introductory portion of nearly 100 pages the antiquity of man in the country is considered. Here the strokes are usually bold enough, but, in the present writer's view, are not always happily directed. The conclusions reached more than a quarter of a century ago are re-affirmed. More recent discoveries have not shaken seriously the foundation of the author's faith. Long ago a sort of algebraic formula was worked out in which, when the substitutions, of whatever value, were duly made,  $x$  always came out with the same value: There are doubtful elements in the case; be cautious; reserve a decision. We are still assured of the accuracy and usefulness of this formula.

Aside from the question of man's antiquity on this continent the author of the work seems to be no more cautious than the average of scientific writers. He appears to be convinced of the essential unity of the human race and of the derivation of the American Indian from Asiatic ancestors; but enough objections might be opposed to these propositions to deter an extremely cautious man from committing

himself. He seems to believe (p. 47) that almost any kind of a primitive people, of however little capability and culture, cast into the Pueblo region, would at length have attained the Pueblo stage of culture; although most people have faith in the precept:—*Mens agitat molem*. It remains to be seen whether the whites who take possession of that region will reach the same stage of civilization. He seems to be convinced that the ancestors of our Indians crossed Bering Strait on the ice and in some way slipped around the Wisconsin glacier and reached the Atlantic coast not long after, if not before, the glacier had retired from our States. In view of the mass of evidence, much of it produced by trained scientific men, to the effect that man was here before the Wisconsin ice period, the author does not appear to employ the cautious methods that he recommends to others.

The doctrine that men lived from sea to sea in Europe during the Pleistocene is accepted (p. 36) without hesitation, because there their bones and artifacts are associated with remains of extinct animals, including the mammoth and the mastodon. Again and again in our own country, bones of men and relics of their handiwork have been found buried in deposits laid down well back in the Pleistocene, or mingled with remains of animals long ago extinct; but on this side of the sea nature is roundly denounced as playing Mephistophelian tricks on us to the extent that her teachings, as regards Pleistocene man at least, are thoroughly discredited.

An argument against man's early presence in our region is derived (p. 38) from the fact that none of his remains have been found in early deposits in the region about Bering Strait. The bear *Ursus americanus* has been identified from early Pleistocene deposits (Aftonian). His forebears undoubtedly arrived *via* Bering Strait. We are hardly obliged to regard the Aftonian as very recent because no bones of this bear have been found in early deposits in Alaska; nor need we believe that the specimen, a jaw, worked its way down into the Aftonian through crevices or gopher holes. We are not obliged to point out a trail of bones all the way back to India and Africa to prove the geological age and the relationship of our American eland to that of Africa. And then even if there had been found in early deposits along Bering Strait a skull of a low-browed human it would probably have been at once clapped on the shoulders of some mythical pilgrim from, perhaps, Borkum isle or Lopperzum.

Paucity of numbers among primitive men is in the way of becoming

an anthropological dogma. It seems to be thought that the first human beings, savage and living from hand to mouth, were as infertile as the prosperous and educated folk of our own time. The way in which the primitive peoples took possession of the world, despite the hindrances encountered, rude climates, mountains, broad seas, deserts, wild beasts, and diseases, shows that they were a vigorous and adaptable race and that they obeyed cheerfully the divine command imposed on the first pair.

The reputed discovery of human remains at Table Mountain, California, is again discussed (pp. 61-68). During a period of thirty or forty years miners continued to report the finding of human remains and artifacts, many of them in tunnels beneath layers of lava. The matter was investigated by various persons, many of them men of education and high intelligence. We are told, however, that these men lacked discrimination or were the victims of their own gullibility and the humorous trickery of the miners. This explanation has been presented so often that our anthropologists evidently believe that joking may become a serious matter. We are now told (p. 67) that the mining camp is the natural home of practical joking. Mining communities have indeed a reputation for occasional wild behaviour; but few of us have supposed that the sending thither of governmental commissions and of armed troops was for the purpose of suppressing exuberant hilarity. It seems that it requires the poetical (Bret Harte) and the artistic temperaments to detect, amid the grosser manifestations of the mentality of miners, the subtle presence of the practical jest. Within our wide-flung boundaries are mines of coal, iron, lead, copper, silver—hundreds of happy homes for practical jokes. Strange it is, that nowhere has this form of human activity furnished any such products, backed by at least plausible evidence, as it did at Table Mountain. Otherwise our museums might have been enriched by human skulls and fashioned flints from all geological formations from the Archaean to the Eocene; unless, indeed, our anthropological curators had been still more strenuous in their efforts to keep their cases clear of specimens that did not add embellishment to their theories (Science, vol. 47, p. 561).

The author of Bulletin 60 appears to be hard to please. He insists strongly that man arrived in America in postglacial times. If, however, his opponents urge an earlier arrival, he insists quite as strongly that this occurred in the Tertiary. *In medio stat virtus.*

We are told (p. 16) that the most serious hindrance to progress toward a correct interpretation of the history of early man in America has been the assumption that the history has run parallel with that in Europe. But that is the theory that grips our anthropologists to-day. Because skeletal remains of a low grade of human beings are found throughout the Pleistocene of Europe it is argued that, if man had existed here during that time, similar skulls and bones would be met with. It does not follow. The study of Pleistocene mammals shows that there has been extremely little change in their structure during that time. Is there any reason why man alone should have made rapid progress? He may have had in the early Pleistocene the same physical, perhaps mental, characteristics that he now has; and he may have entered this continent with the animals that he lived with in Asia; and that even before men of the same grade had entered Europe. The old Heidelberg men and Neanderthal men were possibly imperfectly developed *Hominidae* that had persisted from Pliocene times.

The author reviewed shows signs now and then of wavering in his conclusions. He appears to grant that the Nampa image may after all have been fashioned by Pleistocene hands; he is disturbed by the discoveries of McGee and King; at Table Mountain there are deep gravels that appear to have yielded traces of human occupancy. This is a good little beginning somewhat long deferred. Sooner entered upon, it would have obviated the need for that "strenuous opposition" (put somewhat euphemistically) which has been meted out to various men who may, after all, be shown to have possessed better judgment than the experts.

There is not one who will not wish that the honored author of Bulletin 60 may have yet many years for his useful work.

OLIVER P. HAY.

No. 4—pp. 13-16

January 24, 1921

## ANTHROPOLOGIC SCRAPS.

1211 HARVARD STREET.

WASHINGTON, D. C.

---

### THE NEWEST DISCOVERY OF "ANCIENT" MAN IN THE UNITED STATES.

---

This paragraph is devoted to a gratuitous and unsolicited commendation of the American Journal of Physical Anthropology. Its covers bear many distinguished names. If these names shall often appear within the covers at the head of appropriate articles the publication will fill an important place.

The first number for the year 1920 contains, on pages 187-193, an interesting paper over the familiar initials "A. H." The present writer ventures to touch lightly and with due respect on the subject there discussed.

Out Zanesville-Ohio-way last July that apparently ubiquitous, irrepressible, unholy thing, alleged Pleistocene man, suddenly made its appearance and threw that peaceful community into a state of excitement. The disturbance was not localized, but reached the sensorium of the Department of Anthropology in Washington City. Reaction was instantaneous and powerful. The wires became hot with orders. First the State Archaeologist was summoned to the scene; but that too industrious official was busy at some distant point when he ought to have been lying around ready to smother such eruptions. There was a brief delay; but soon the electrical crepitations and coruscations recommenced, and they continued until a geologist was discovered and induced to repair to the point of danger. So imminent was the affair that A. H. himself offered, if the case seemed to warrant it, to be on hand "on Tuesday evening."

What had happened was, according to the subsequent report of the geologist, about as follows:

Along Muskingum River there is an extensive and long-worked gravel pit. The face of this pit is about 75 feet high and nearly vertical. Thirteen feet from its summit there is a somewhat harder layer which gives rise to a sort of shelf. Now, right on this shelf this pro-

team impostor had sprawled himself out in the form of a nearly complete human skeleton. That, however, had happened some little time before the real trouble began; and when the geologist got there that shelf, like another in a juvenile tale, was bare. Whether the skeleton before disclosure had been lying at a depth of 15 feet (including 2 feet of surface stripping) or had slumped down from above could not be determined. If it had lain at the depth indicated, the geologist thought that this did not exclude intentional burial.

A skull in due time reached the hands of our official physical anthropologist and was promptly pronounced to be that of an aged woman, probably Indian. One must contemplate with admiration the number and the curious construction of the instruments of precision employed by our anthropologists in taking measurements of the human skull; also the ingenuity displayed in securing dimensions of all parts and in all directions; still more the classical formation and sonorousness of the terms applied to these parts and dimensions. But when it comes to getting a decision, the result is sometimes disappointing. In the case of the skull found at Vero, Florida, our physical anthropologist could not shake himself free from the feeling that it had belonged to a white man, or at least to one half-white. In another notable case, to Dutch dunderheads spawned in the marshes along the shores of the German Ocean were imputed two chamaecephalic pates that had been unearthed on the plains about Trenton, N. J. So again at Zanesville the possibility was not excluded that the aged dame had belonged to the white race. It would be ungracious indeed to insinuate that this Caucasian affinity had been suggested by that tenuous tale about certain stones, standing near that spot 40 years ago, set up "after the manner of placing tombstones," and bearing certain mystical characters that no one had ever been able to read.

We are not informed what precautions were taken to determine the authenticity of the skull of the aged woman. Too well we know the pitfalls that lie in the path of the scientist. Now did that skull belong with the skeleton that was found on that shelf? May not the one have slumped down from above and the other have been buried in the glacial valley train? There are reasons for suspecting this. The man who dug up the skeleton stated that the body was 6 feet long. It must therefore have belonged to a large man; the skull is that of a small woman, "hat band measure 14 inches." After the skull was found it was for some time viewed and handled by many persons. Is it not very possible that during this period some joker

or some trickster substituted for it another much like it? Is the one that the physician examined the one received by the Department of Anthropology? There are evident discrepancies in the two descriptions. Many strange things occur during the transit of objects; may not the skull put by the State Archaeologist of Ohio into the care of the express company have been abstracted by some lover of gruesome curios? We shall try to feel at ease with the thought that the physical anthropologist has with his usual vigilance traced the history of the skull *ab initio*. Nevertheless a presentiment oppresses the writer that 40 or 50 years from now, when their ebon or ruddy locks shall have been replaced by shiny scalps, when their whiskers shall have grown long and white, and their eyes become bleary, the shovellers of that gravel pit will come up and boast of the trick they worked off on our confiding physical anthropologist. The writer has in his time seen many sunny sandbanks and tarried in many a gravel pit; and he can affirm that they, and not gold mines, are the real homes, the very nurseries, of practical jokes.

With no desire to disparage the services of the inspecting geologist, the writer personally regrets that that scientist was available. It would have been so much more satisfactory if the official physical anthropologist could have been personally present, instead of administering absent treatment. He has himself lamented the fact that such cases are not placed for investigation first of all in the hands of the anthropologists. His own qualifications have been proved in more than one difficult case. To wit: Some amateur anthropologists in Nebraska had found human bones buried in loess, and they concluded that this meant the existence of man there during the period of deposit of the loess. If now the reader will peruse Mr. Robert F. Gilder's article in *Records of the Past*, volume 10, pages 157-169, he will learn how much excellent geological work can be accomplished in a half hour by an enthusiastic physical anthropologist equipped with suitable tools; also with what accuracy he can report facts, statements, and geological and anthropological conditions. The present writer is glad to add his own testimony, if it is needed, having watched the development, by the official referred to, of a geological section along the canal at Vero, Florida. This section was 60 feet long and was characterized by its excavator as being both comprehensive and illuminating. It is, however, to be regretted that up to the present the pressure of official duties has prevented my colleague from telling us what it comprehended and what it illuminated.

If now a similar section some scores of feet in length had been run along the aforesaid shelf at Zanesville, our honored anthropologist might possibly have found some other bones of the venerable little woman, or bones of the large man, or those of some other human being. His intimate knowledge regarding the behavior of human bones after burial and his experience with the treacherous nature of unconsolidated clastic deposits would have enabled him to determine to which of the following four categories the relics belonged: (1) Remains which had recently been buried near the surface and which had been brought down by slumping. (2) The skeleton of some tough citizen whose body his exasperated fellows, taking no further chances, had buried at a depth of 15 feet. (3) Bones which had been superficially buried, but which had, as is known sometimes to happen, migrated to a lower level. (4) Remains which had been buried there in the valley train when the glacier was not far away.

Had our anthropologist and geologist been able to demonstrate that the case came under category 4, he might have found himself acclaimed Father of Pleistocene Man in America, all competitors having been duly and justly disqualified and discredited.

At the close of his dissertation A. H. emits a note of relief that by prompt action the affair had not become one of the ambiguous cases that have so much worried him. It is certainly only people who have some dislocation in their reasoning faculties who will insist that, inasmuch as the inspecting geologist could not be sure that that skeleton was not buried in the glacial outwash, it is exactly an ambiguous case. However, one can not please everybody; and our author ought to feel encouraged to attack the results obtained by the American Museum on the river plain at Trenton, New Jersey. He might quickly repair the damage that in the estimation of many people has been wrought on the theories of the Old Guard, the confirmed foes of Pleistocene man. He might even tell us why it is that these appearances of supposed Pleistocene man occur at localities where the environment would have permitted him to exist and not at other places and under conditions where he could not have existed.

OLIVER P. HAY.

## ANTHROPOLOGIC SCRAPS

1211 HARVARD STREET.

WASHINGTON, D. C.

---

### THE PEOPLE WHICH SAT IN DARKNESS SAW GREAT LIGHT.

---

The scientific writings of Dr. Ales Hrdlicka are always a source of joy, of instruction, and of inspiration. For the cultivation of the ratiocinative faculty they are the equivalent of a university course in logic. It is, therefore, a pleasure to review a reprint entitled "The Anthropology of Asiatic Peoples."

We must count it fortunate for those who dwell in the shadow and darkness of heathen lands that a religious or a scientific apostle, anointed or self-appointed, bearing only a message and a hand-bag, can command railway trains and steamships and can follow, perhaps in the very footsteps of the renowned Phileas Fogg, Esq., but with still greater speed; that there need be no interruptions in his journey, as there were in Fogg's case, and hence no compulsion to buy and burn any ships or to add to his impedimenta any widow or any elephant; and that at last he can discharge his intellectual or spiritual burden and return without having been missed from his accustomed haunts.

The contents of the modest brochure were delivered before a joint conference of two associations in China, the Medical Missionary and the National Medical, at Peking, in 1920. The reader shall not be wearied with the details of the weighty problems discussed in the pamphlet; but certain "high lights of the question" were touched upon which invite attention.

It is refreshing in these days when the classical literatures are neglected, and even spurned, to find a scientist who, in the midst of a discussion of the origins and distributions of the races of our poly-chrome humanity, of their different "physical, chemico-physiological, mental and pathological characteristics," can for a moment turn lightly aside to those stories which have charmed the intellects and

hearts of men ever since prehistoric times; who can, in fact, show that he is as well versed in mythologic lore as he is in paleontology, geology, or even, perhaps, in anthropology. So our distinguished traveller, to encourage them to greater activity in anthropologic work, blithely assured those religious and medical carpetbaggers over in China, good people all, that "southern Asia is a great Pandora box, in which there is doubtless hidden many a precious gem of information, and the time is nearing when this box, too, will be made to open to science." This charming interpretation of the story can, however, hardly have been obtained from Hesiod. Pandora herself must have handed it out.

Our author is sure that modern man has descended from the "still exceedingly primitive and semi-anthropoid man of Heidelberg," of about the middle of the Pleistocene. Inasmuch as this proposition has not passed beyond the stage of discussion, one may be allowed to examine its probability.

In probably no family of mammals has there been made, since the beginning of the Pleistocene, any considerable evolutionary progress. Matthew has estimated it at about 10 per cent of that made during the Pliocene, a liberal allowance. The horse of the early Pleistocene does not stand midway between the horse family and some other; but is so truly horse that often his remains can hardly be distinguished from those of the existing equine. Nor is the brain smaller, that organ which in man is supposed to have undergone so great increase in size. The same statement may be made regarding the elephants, the bisons, the cats, the wolves, etc. Many yet living species are recognized in collections made in early Pleistocene deposits. It would be unsafe to say of any existing species of mammal that it was not already in existence at the beginning of the Pleistocene. Is it not then probable that man was in the early Pleistocene about what he is now? Is it probable that he has made more progress during one half of the Pleistocene than any other mammal has made during the whole of it? If our physical anthropologist believes so it is up to him to prove it. The present writer believes that the old Heidelbergers and the Neanderthalers, together with the cultures associated with them and their like, are a lot of left-overs from the Pliocene; and that the anthropologists, in guiding by them their course in human evolution, have run into a *cul-de-sac*. Somewhere in Asia, about the end of the Pliocene or a little later, the most advanced races of men had probably reached the stage now held by our more backward

races and had already passed through the stage of paleolithic culture. Doctoer Hrdlicka might consider whether or not this is the reason why no paleolithic implements have been found in Japan; in case it is true that none have been found.

Doctor Hrdlicka still nurses the hypothesis that early Pleistocene men were *rarae aves*. Hardiness, enterprise, and fertility were probably the qualities which gave our Pliocene and Pleistocene ancestors their dominance. The era of comparative infertility was far in the future. No colleges had yet been founded for women and no eugenic societies for men.

Our author is insistent that the earliest immigrants into America had been driven by the struggle for existence to the inhospitable northeastern corner of Asia; and that from there, a few thousand years ago, they made their way slowly and painfully, perhaps in little boats, to the new continent. He ought to know that the geography, the topography, and the climate in these regions have not always been what they are now. In probably the early Pleistocene, possibly also as early as the late Pliocene, a way was opened up for travel by *terra firma* between Asia and America. Geologists recognize that there was then a period of great elevation of our continent. The Cascade Range, as an example, began to be uplifted during the early Pleistocene.

We are compelled on various accounts to suppose that one or two bridges were at that early time thrown across Bering Sea. A barrier extending between East and Prince of Wales capes would have shut off the cold current coming down from the Arctic Ocean, as a result of which the sea and the land southward would, through the influence of the Japan current, have become warmer. Along the north side of this barrier the boreal animals might have crossed over; while those of temperate Asia might have made their way on the southern side. As a matter of fact, early in the Pleistocene there swarmed into America elephants, bisons, musk-oxen, goats, bears, wolves and foxes, besides other mammals of smaller size. These surely were not all inhabitants of a boreal climate. If, now, elephants and ground squirrels could make the passage, man could do the same; and the journey may have been a very pleasant one.

Our lecturer feared that some of his hearers had learned of discoveries of early man in America and he hastened to tell them that "every report of that nature, when properly investigated, had been found to be erroneous." That depends, however, on whose investi-

gations he had in mind. If his own, nobody will question his statement; it will be conceded in advance in any future case. If, however, he refers to investigations made by those who have opposed most consistently the notion of Pleistocene man in America, then the "erroneousness" is wholly his own. In very few cases have those men claimed that it was proved that the find under investigation did not belong to the Pleistocene. They have contented themselves with holding that the proof was not sufficient to demonstrate this age. Doctor Hrdlicka ought to consult his former chief on this matter.

Our physical anthropologist has too often been unfortunate in reporting the views and sayings of other people. In another case (*Records of the Past*, Vol. X, p. 158) the charge of misrepresentation was made against him, and he confessed judgment. In reporting on the case at Vero, Florida, he wrote that Mr. Weills had told him that certain bones had been found lying in "their natural relations." Mr. Weills has always protested that he imparted no such information; furthermore, that other statements about these human bones were "erroneous." However, to err is human. Doubtless the intentions of our physical anthropologist were good. The errors committed may once more be attributed to "hastiness of preparation." It may be mentioned in passing, as a curious coincidence, that these chance deviations from accuracy seem always to favor Doctor Hrdlicka's side of the question.

A tribute has already been rendered to the logical powers of the author here reviewed. Confirmatory of the justness of this an incident may be cited: In 1916 (*Jour. Geol.*, Vol. XXV, p. 49) Doctor Hrdlicka visited Demere Key, off the west coast of Florida. Years before this, some interesting Indian structures had been found here. The age of these is wholly unknown. Doctor Hrdlicka concluded that their age was possibly post-Columbian. "For there were found on the Key fragments of Spanish pottery and glass, while burial sand mounds on neighboring keys yielded glass beads." It will furnish the reader a pleasant and possibly profitable exercise to construct the syllogism which underlies this reasoning. Who, however, can, in this case again, question the benevolent intentions of our physical anthropologist?

OLIVER P. HAY.



7 DAY USE  
RETURN TO

RETURN TO  
**ANTHROPOLOGY LIBRARY**

This publication is due on the LAST DATE  
and HOUR stamped below.

RB17-30m-10.74  
(S1664L)4188

General Library  
University of California  
Berkeley

GAYLAMOUNT  
PAMPHLET BINDER

Manufactured by  
GAYLORD BROS. Inc.  
Syracuse, N. Y.  
Stockton, Calif.

EL 1251329

